a large long mass of sand, suddenly giving way and falling forward from the vertical face of a sand-bank or river-bank on to the water, will produce, and the very long distance it will be heard up and down the river.

In the vicinity of such great rivers, at such a spot as Chilmari Ghât, which I know well, we must be rather sceptical with regard to sounds being any other than of this nature. The reports are very likely to be repeated, for the wave produced on the water will set another mass falling some way off, and even the wave of air may do the same to a mass just tottering, and bring it all down together (see also Report in *Proceedings* of Asiatic Society, Bengal, p. 207, regarding the echo from one side of the river to the other, so well described by Mr. Manson).

Mr. W. T. Blanford has suggested to me landslips for the reports heard near Buxa; but this is a sound I have heard on the bursting of a landslip dam in the Himalayas: the sound was of the nature of a rumble, not a report. A fall of rocks into a valley from any height would produce, I think, a still more prolonged rumbling sound—more like that of the avalanche.

Shalford Park, January 7. H. H. GODWIN-AUSTEN.

ALLOW me to mention, for the information of those of your readers who take an interest in this subject—a considerable number, to judge from the correspondence that has lately appeared in these columns—that a very complete account of the phenomenon is now being published in *Ciel et Terre*, a review devoted to astronomy, meteorology, and terrestrial physics, published twice monthly at Brussels, from the pen of M. Ernest Van den Broeck, curator of the Royal Museum of Natural History of Belgium, who has devoted much time and labour to a study of the subject. The first article, containing a complete historical review of the subject, appeared on December 1; the second, on the 16th, dealt with the accounts of the phenomenon received by M. Van den Broeck from various observers in Belgium; and future articles will be devoted to a study of the facts as reported from Bengal, to the causes of the phenomenon, and to the methods employed in determining its origin.

I may mention that the true rendering of the word *mistpoeffers*, the name given to the sounds by the fishermen, is not *fog-dissipators*, but, as appears from a note on the etymology of the word by one of M. Van den Broeck's correspondents, it should more correctly be translated *fog-belchings* or *fog-hiccups*, the French rendering of the word *poeff* being *renvoi* or *hoquet*. The point is of some importance, since the sounds do not appear to have any effect, as one might imagine from the signification of the name, as it first appeared in NATURE, in the dispersal of fogs. T. D. LA TOUCHE.

Boring a Coral Reef.

WILL you be good enough to allow me to correct a somewhat important oversight in the note in last week's NATURE, on the work of the Coral Reef Committee of the Royal Society. You mention that the Royal Society has granted the sum of £800, and the Government a gunboat, for the expedition. Even with this help, however, it would have been impossible to undertake the work unless the Department of Mines of the New South Wales Government, largely through the good offices of Prof. Anderson Stuart, had granted to the Committee the use of a complete set of boring tools and appliances, with an engine and boiler. Further, the Department has relaxed the somewhat stringent restrictions usually placed on the use of the tools, owing to the difficulties which will be experienced in a waterless and sparsely inhabited island. For such generous assistance, and to Prof. Stuart, who has helped in many different ways, the Committee cannot be too grateful. W. W. WATTS,

(Joint-Secretary of the Royal Society's Coral Committee).

Sutton, Surrey, January 11.

Variability of Red Stars.

ACCORDING to a notice, which I found in NATURE'S "Astronomical Column" of November 14, 1895, my theory of the variability of red stars should demand, that the bright lines of hydrogen should be produced there at a low temperature and in a condition never tested by experiments.

I will try to show that this objection is not valid.

The bright lines in red stars are considered by me as caused by the recently much studied phenomenon of luminescence, *i.e.* by a production of light not due to elevation of temperature, and thence not subjected to Kirchhoff's law of absorption.

In the cooled atmospheres of the red stars two causes of luminescence can be expected—two causes, which according to the relatively low temperature of the combinable elements and condensable vapours, with which these atmospheres are filled up, must act there much more energetically than in other stars. These two well-studied causes of luminescence are chemical action and electricity.

If we first consider chemical action, it may be stated, by the way, that the supposed chemical combination in a cooling star does not necessarily require a very low temperature. Acetylene, for instance, can be generated in the electric arc. Now we know that acetylene is a compound actually present in the atmospheres of some of the most cooled stars. It is abundantly absorbing in the stars III *b* and, according to Prof. Lockyer, not unlikely radiating in the stars of his Group II. (Vogel's Class III *a*).

If therefore we assume that it is principally acetylene, which is generated in the cooling atmospheres of the red stars, bright lines of hydrogen may be expected there. These lines are then precisely caused as those of sodium, potassium, lithium and thallium in the experiments of Pringsheim, being absent as long as the sodium vapour is only strongly heated, but appearing immediately as soon as in this heated vapour some chemical change occurs.

It must be conceded, however, that in the case of hydrogen this chemical luminescence has not yet been actually observed in our laboratories. But in the case of hydrogen we have another luminescence, which is daily experienced. It is the glow in Geissler tubes, where (whatever may be the heat of the individual shining molecules) the average temperature of the glowing gas does not necessarily exceed the common temperature of our atmosphere, and cannot therefore be considered as the cause of the glowing of the gas.

As this luminescence is caused by electric discharges, and as such discharges (according to what we know about the electrical phenomena connected with the formation of clouds and hail in our own atmosphere) are likely to be expected in atmospheres, which are filled up with vapourous matter ready to condense in clouds, the hypothesis of an electrical luminescence of the hydrogen in the atmospheres of red stars seems very plausible. That hypothesis demands that the bright lines should be especially conspicuous in stellar atmospheres, where the alternation of vapourisation and recondensation is also very conspicuous, *i.e.* (according to my theory explained in the November and December numbers of *Knowledge*) in the atmospheres of red variables and Novæ, where that intermitting condensation and vapourisation of dark obscuring cloudy matter is the very cause of the variability. Now we know that this demand is fully verified. Prof E. C. Pickering has stated as a rule that, with perhaps a single exception, every red star with bright hydrogen lines is *eo ipso* variable. And this rule has proved to be so sure, that Mrs. Fleming could discover numerous new variables from the bright lines of their spectra (*Astrophys. Journal*, I. p. 27, 411; II. p. 198).

Eventual bright lines in the stars of Vogel's Class I. and II. may be explained perhaps (as Prof. Scheiner has recently shown in his "Untersuchungen über Spectra der Helleren Sterne," p. 223) by the hypothetical presence of a gigantic incandescent atmosphere, whose radiation around the bundle of rays coming to us from the star's much smaller photosphere is greater than the absorption it causes in that bundle; but in the more cooled atmospheres of red stars the chemical compounds there present, attest a temperature relatively so low that we cannot conceive that atmospheres to be filled up to such a gigantic height with hydrogen so enormously heated as to become (if that still doubtful phenomenon is possible) bright shining by incandescence.

Such enormously heated hydrogen and chemical compounds cannot permanently coexist in a stellar atmosphere. The coexistence of chemical compounds and bright shining hydrogen is only possible if the brightness of the latter is due to luminescence.

The ideas here suggested may be considered as an instance of a likely fruitful application of the study of luminescence to stellar spectroscopy. If they are right, they give, I think, a plausible explanation (1) of the connection between the variability of a star and the frequent brightness of its spectral lines, and (2) of the very remarkable fact that both variability and bright lines are so often observed precisely in those stars, whose

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atmospheres, according to the chemical compounds or their spectra, are coolest. A. BRESTER.

Delft (Holland), November 30, 1895.

THE above amplification of Dr. Brester's views, as to the cause of variability in red stars, calls for a few remarks. Although opinions may differ as to the constitution of stars of Group II., it is generally agreed that those of Group VI. are in an advanced state of condensation, with cool, absorbing atmospheres, and it is in the latter group of stars that we find the full development of the fluted absorption spectrum which Dr. Brester ascribes to acetylene. Experimental evidence does not certainly indicate that this gas is responsible for the dark flutings observed, for other compounds of carbon give a similar spectrum; but, to whatever the flutings may owe their origin, they are a result of Kirchhoff's law. Notwithstanding that it is in these stars of Group VI. we should most expect the bright lines on Dr. Brester's theory, no bright lines have been recorded in variable stars belonging to the group; that is, there is no visible chemical luminescence.

It is in the variables of Group II. that the bright line phenomena occur, but the associated dark flutings of metals, or their compounds, do not lead us to suppose that the temperature is lower than in the other group of stars with fluted spectra. Hence, luminescence does not seem to be more probable in Group II. than in Group VI., unless we accept the view put forward by Mr. Lockyer, that the physical constitutions of the two groups of stars are essentially different. If the stars of Group II. consist of uncondensed swarms of meteorites, it is certainly conceivable that the luminosity of the hydrogen in the interspaces may be partly due to electrical excitation; this view involves the supposition that these stars are becoming hotter.

It may be further remarked that there is abundant experimental evidence to show that the line spectrum of hydrogen can coexist with a fluted spectrum. Acetylene, for instance, exhibits such a mixture, and hence luminescence is not more necessary to explain the luminous phenomena in the case of hydrogen than in the case of the absorbing vapours which give the flutings associated with them in stars.

THE WRITER OF THE NOTE.

Mount Wosho.

AS my name has been mentioned in NATURE for December 5, 1895 (p. 107), in connection with recent explorations in Africa, allow me to say that snow clothing on Mount Wosho must have been added by an outsider, for in page 110 of my work on Ethiopian geography, published five years ago, I have expressly said that snow does not exist in that country. In page 387 of the same volume, I have given up all Mount Wosho's claims to an immense height; but I still think it exists as a mountain, and that it ought not therefore to be wiped off the map.

Your criticism induces me to explain how I tried to sketch one in Upper Ethopia. I first employed the common method by recording hours of travel and bearings by compass; but I was then forced to suppose the variation of the needle. Disturbing attractions could not be eliminated, and it was seldom possible to check my road by observed latitudes. However, as a warning to my successors, I published this tedious work in my "Géodésie d'Ethiopie" (Paris, 1873), a volume of 502 quarto pages.

This first attempt to map the country being fruitless, I turned all my attention to using a theodolite, and collected in 318 stations more than 4000 bearings, besides 500 of the sun, taken at proper hours to get true azimuths, each of these being followed by an angular zenith distance. I obtained three base-lines from differences of latitudes observed on two heights situated near the same meridian and connected by azimuths. The northern base is nearly 51 miles long; I got there my longitude by eight occultations of stars. The second base is 52 miles calculated from a quadrangle, and the southern base, still more indirect, reaches 51 miles. I have thus carried a continuous chain of triangles from the Red Sea to the frontier of Kaffa, a distance slightly greater than from Calais to Bayonne. These triangles, mostly with only two observed angles, have been checked here and there by latitudes and independent longitudes. I got the latter by a few occultations, but chiefly according to the Russian method of lunar observations. By this method I have calculated 857 positions in Ethiopia. They are given in my pages 423-440, the heights being obtained by a *supposed* coefficient for terrestrial refraction.

In my "Géodésie" (page 195) I give the first mention of Mount Wosho as *probably* Wosho, and the following page says, "flat mountain which I suppose (*que je prends pour*) Wosho." Page 438 points in No. 805 to a note (p. 448) which adds, "placé pur renseignements et par ces azimuts réitérés." This hearsay evidence is taken from a list of days' journeys between Bonga and Wosho (published in my "Géographie") without details. The profile of the mountain measured from Falle is given in the collection of sketches belonging to my 325 *tours d'horizon*. Moreover, the zenith distance was 90° 21′, nearly the very worst to be corrected by the ordinary rule for terrestrial refraction. In spite of all these drawbacks, I published the resulting enormous height in order to call attention to Wosho. Let us hope that some explorer, after throwing a net of triangles over Walamo, may put in its proper place and height the real Wosho.

ANTOINE D'ABBADIE.

Hendaye, December 13, 1895.—

I AM aware that M. d'Abbadie did not still maintain the exaggerated importance once attached to Mount Wosho, for which, indeed, he is by no means solely responsible. My remark was suggested by the fact that the great mountain reported by Grixoni's expedition had again called attention to Mount Wosho, and led to the idea that the earlier reports were possibly, after all, correct. But until Dr. Donaldson Smith's maps are published, it is no use attempting to reconcile the previous maps of this region. THE WRITER OF THE NOTE.

Cactaceæ in the Galapagos.

ALTHOUGH the Editor has kindly replied to that part of Mr. Agassiz's letter [*ante* p. 199] relating to the *Albatross* Expedition, I should like to say a few words on the other points raised by him. In the first place, I regret that my communication on the subject should have produced the impression of unfairness towards any person or persons. Such was not my intention. For the sake of my own reputation, as well as for the information of those who, like Mr. Agassiz, might, from my way of expressing myself, think otherwise, I may add that I did not intend to convey the idea that *Opuntia* and *Cereus* are limited to Chili on the west coast of America. The contrary is so well known, that it did not appear necessary to me to enter into particulars. Of course Mr. Agassiz will hold me guiltless of suppressing anything respecting the branches of *Opuntia* and *Cereus* which he says he collected, though he does not know what became of them.

Perhaps I may also be permitted to add that Dr. Baur has since sent me some very fine photographs of *Opuntia* and *Cereus*, on a comparatively large scale, together with some notes, which I laid before a recent meeting of the Linnean Society.

Herbarium, Kew. W. BOTTING HEMSLEY.

A Luminous Centipede.

IN Mr. Lloyd Bozward's letter on the above subject in this week's NATURE, he says that the light of the *Scolopendra electrica* "is the same as that of the glow-worm." My experience is that the light of the glow-worm is a clear little *spark* of light; whereas that of the *Scolopendra electrica* (which I have usually found in the autumn) is more of a phosphorescent light, and streaks of this light are left for a few seconds in the trail of the animal as it crawls about, so that it is often difficult to say exactly where the creature is. I have no doubt others have observed the same thing. T. PLOWMAN.

Enfield, N., January 11.

The Critical Temperature of Hydrogen.

WHAT I object to is not Mr. Bryan's reference to Wroblewski's work, but his statement (explicitly founded upon the absence of "fresh experiments") that my conclusions "are not results of independent original investigation." In his letter to NATURE of January 9, Mr. Bryan does not even attempt to justify this statement. My *Bulletin* paper was an abstract; in the full paper (*Trans. Crac. Acad.*, vol. xxvii. p. 375), published May 1895, Wroblewski's work was quoted and discussed.

Cracow University, January 12. L. NATANSON.

A Fog Scale.

CONSIDERING the important part that fog plays in determining the character of a health resort, it is remarkable that no

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